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THE GRAVITATIONAL RED SHIFT AS A TEST OF GENERAL RELATIVITY: HISTORY AND ANALYSIS

CHARLES St. John, who was in 1921 the most widely respected student of the Fraunhofer lines in the solar spectra, began his contribution to a symposium in *Nature* on Einstein's theories of relativity with the following statement:

> The agreement of the observed advance of Mercury's perihelion and of the eclipse results of the British expeditions of 1919 with the deductions from the Einstein law of gravitation gives an increased importance to the observations on the displacements of the absorption lines in the solar spectrum relative to terrestrial sources, as the evidence on this deduction from the Einstein theory is at present contradictory. Particular interest, moreover, attaches to such observations, inasmuch as the mathematical physicists are not in agreement as to the validity of this deduction, and solar observations must eventually furnish the criterion.[1]

St. John's statement touches on some of the reasons why the history of the red shift provides such a fascinating case study for those interested in the scientific reception of Einstein's general theory of relativity. In contrast to the other two 'classical tests', the weight of the early observations was not in favor of Einstein's red shift formula, and the reaction of the scientific community to the threat of disconfirmation reveals much more about the contemporary scientific views of Einstein's theory. The last sentence of St. John's statement points to another factor that both complicates and heightens the interest of the situation: in contrast to Einstein's deductions of the advance of Mercury's perihelion and of the bending of light, considerable doubt existed as to whether or not the general theory did entail a red shift for the solar spectrum. Even the ablest expositors of the theory seemed unable to give a clear and cogent derivation of the 'Einstein effect'; indeed, our search of the literature has not turned up a single unproblematic presentation of the correct formula for the red shift prior to the mid 1920s. Many competent physicists naturally found grounds for objecting to the purported derivations, and less competent ones found in them an invitation to raise muddled arguments against the theory. The discussions that followed did not always illuminate the theory, but

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[1] C. E. St. John, 'The Displacement of the Solar Lines,' *Nature*, **106** (1921), 789 – 790; quotation from p. 789.

Stud. Hist. Phil. Sci., Vol. 11, (1980) No. 3, pp. 175 – 214.
Pergamon Press Ltd., Printed in Great Britain.

goal. But it is exactly the heuristic fruitfulness of the principle of equivalence that tends to obscure what the final theory says about the red shift. Many attempts have been made to remove the looseness and vagueness of the formulations of this principle that preceded the general theory of relativity and to find some precise counterpart within the completed theory;[2] but there is still much disagreement on this matter, and in almost all of the attempts, the precision is purchased at the price of the original heuristic power — here, as in many other cases, a certain amount of imprecision and vagueness seems indispensible. Our task, however, is not that of assessing the validity of the principle of equivalence in the light of subsequent developments, but rather that of describing how Einstein used it and how this use affected attitudes towards the red shift.

The fifth and final part of Einstein's 1907 essay on the principle of relativity and its consequences gave itself over to speculations about a question which 'forces itself. . . on the mind of anyone who has followed the previous applications of the principle of relativity'; namely, 'Is it conceivable that the principle of relativity also holds for systems which are accelerated with respect to each other?'[3] Einstein argued for a positive answer, at least for the case of uniform acceleration: as far as we know, the laws of physics are the same for two systems Σ_1 and Σ_2, where Σ_1 is accelerated in the direction of its x-axis with acceleration γ, while Σ_2 is 'at rest' but situated in a homogeneous gravitational field characterized by a gravitational acceleration of γ along its negative x-axis.

> We have therefore no reason to suppose in the present state of our experience that the systems Σ_1 and Σ_2 differ in any way, and will therefore assume in what follows the complete physical equivalence of the gravitational field and the corresponding acceleration of the reference system.
>
> This assumption extends the principle of relativity to the case of uniformly accelerated translational motion . . . The heuristic value of the assumption lies therein that it makes possible the replacement of a homogeneous gravitational field by a uniformly accelerated reference system, the latter case being amenable to theoretical treatment to a certain degree.[4]

Einstein's 1907 theoretical treatment of uniform acceleration was fruitful in yielding, in combination with the equivalence principle, the consequence that 'light coming from the surface of the sun. . . possesses a wavelength that is greater by about a two-millionth part than that of light generated by identical material on the surface of the earth.'[5] The treatment itself, however, was uncharacteristically cumbersome, principally because of the circuitous

[2]See H. Ohanian, 'What is the Principle of Equivalence?', *Am. J. Phys.*, **45** (1977), 903 – 909.

[3]A. Einstein, 'Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen,' *Jb. Radioakt. Elektronik*, **4** (1907), 411 – 462; quotation from p. 454; trans. from H. M. Schwartz, 'Einstein's Comprehensive 1907 Essay on Relativity, Part III,' *Am. J. Phys.*, **45** (1977), 899 – 902.

[4]Einstein, Ref. 3, p. 454; trans. from H. M. Schwartz.

[5]Einstein, Ref. 3, p. 459; trans. from H. M. Schwartz.

explanations of the concepts involved. Despite the explanations, some of the meanings remained obscure. Planck complained that Einstein's concept of uniform acceleration needed clarification since the rate of change of the (three) velocity is not a relativistic invariant. In an addendum published the following year, Einstein replied than the acceleration of a body is to be measured in at 'acceleration-free' (*i.e.* inertial) frame relative to which the body is instantaneously at rest.[6] While this reply captured one of the key features of the correct relativistic treatment of uniform acceleration, a final clarification was achieved only through the work of Minkowski[7] and Born.[8] Nor did Einstein's reply do anything to clarify the meanings of the concepts of 'time' and 'local time' used to describe the accelerating frame and to derive the conclusion that the rate of atomic clocks is affected by gravity in such a way as to give a red shift.

It is not surprising then that when four years later Einstein again took up the question of the influence of gravity on the propagation of light, his essay opened with the remark that 'I return to this theme because my previous presentation does not satisfy me. . .'[9] This time Einstein's strategy was to sidestep the problems of a detailed analysis of accelerated motion and to exploit instead a coupling of the Doppler principle with the equivalence principle. Let the receiverS_1 and the source S_2 be at rest in the inertial frame K in which there is a homogeneous gravitational field of acceleration γ in the negative z-direction (see Fig. 1). In order to deduce the relation between the

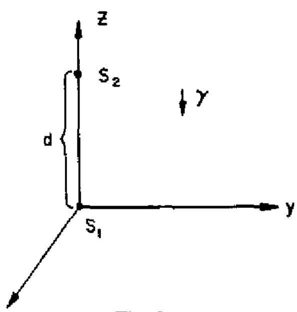

Fig. 1.

frequency ν_2 of a light signal when emitted at S_2 and the frequency ν_1 as measured at S_1, we can imagine a physically equivalent system K' which is

[6]A. Einstein, 'Berichtigungen zu der Arbeit "Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen"', *Jb. Radioakt. Elektronik,* **5** (1908), 98 – 99.

[7]H. Minkowski, 'Raum und Zeit', *Phys. Z.,* **10** (1909), 104 – 111.

[8]M. Born, 'Die Theórie des starren Elektrons in der Kinematik des Relativitätsprinzips,' *Annln. Phys.,* **30** (1909), 1 – 56.

[9]A. Einstein, 'Über den Einfluss der Schwerkraft auf die Ausbreitung des Lichtes,' *Annln. Phys.,* **35** (1911), 898 – 908; quotation from p. 905; trans. from W. Perrett and G. B. Jeffrey, *The Principle of Relativity* (New York: Dover Publications, 1922).

gravitation-free and which moves with uniform acceleration γ in the direction of the positive z-axis to which S_1 and S_2 are rigidly attached. At the moment of emission of the signal from S_2, let K' be instantaneously at rest with respect to an inertial frame K_0. Then at the time of arrival of the light signal, S_1 has a velocity $(\gamma d)/c$ relative to K_0, so that by the Doppler principle

$$\nu_1 = \nu_2[1 + (\gamma d)/c^2]. \tag{1.1}$$

Using the equivalence principle again to transfer the result back to K, we can rewrite (1.1) as

$$\nu_1 = \nu_2(1 + \Phi/c^2) \tag{1.2}$$

where Φ is the gravitational potential.

Einstein was quick to note that this Doppler argument seemed to involve an 'absurdity'. For 'if there is a constant transmission of light from S_2 to S_1, how can any other number of periods per second arrive at S_1 than are emitted at S_2?' His resolution employed an idea already broached in his 1907 paper:

> . . . the two clocks in S_1 and S_2 do not both give 'time' correctly. If we measure time in S_1 with a clock U, then we must measure time in S_2 with a clock that goes $1 + \Phi/c^2$ times more slowly than a clock U when compared at one and the same place.[10]

This way of avoiding the absurdity seemed to imply that one of the basic tenets of the special theory of relativity — the constancy of the velocity of light — had to be abandoned when gravity is taken into account. Einstein embraced this consequence and made it the basis of a further prediction, the bending of light passing near a massive body. Though the predicted value for the deflection of light by the Sun was only a half of the final general relativity value, the existence of the deflection effect was crucially important in providing Einstein with a selection principle for judging the acceptability of theories of gravitation. The notion of a variable speed of light was also the basis on which Einstein attempted to construct a theory of the static gravitational field in 1912; but before turning to this development, another aspect of his 1911 paper needs examination.

The second section of Einstein's 1911 paper focuses on energy considerations; again, the focus is provided by the lens of the equivalence principle. Using the results of special relativity, Einstein concluded that for the construction in Fig. 1, the relation between the energies is, to first order,

$$E_1 = E_2(1 + v/c) = E_2[1 + (\gamma d)/c^2]. \tag{1.3}$$

[10]Einstein, Ref. 9, p. 905; trans. from W. Perrett and G. B. Jeffrey.

The equivalence principle was then used to rewrite (1.3) as

$$E_1 = E_2(1 + \Phi/c^2). \tag{1.4}$$

If the quantum relation $E = h\nu$ had been used at this point, the red shift formula (1.2) would have been derived. Or alternatively, the combination of (1.2) and (1.4) could have been used to argue that if a frequency is to be associated with a quantum of energy, then the energy and frequency must be proportional. But Einstein did neither of these things, and it might seem a little puzzling that he did not. It may be, of course, that since the derivation of the red shift formula was beside the main point of the section, which was to show that from the equivalence principle one could obtain the equivalence of gravitational and inertial mass, Einstein did not want to introduce an irrelevancy. More likely, Einstein did not want to contaminate his work on gravitation with the almost universal skepticism with which his light quantum hypothesis had been greeted; in other publications of this period he tended to keep relativity theory separate from quantum considerations.[11]

In any case, it is clear in retrospect that there are problems in applying Einstein's equivalence principle to the light quantum. Einstein wanted to attribute the excess in energy arriving at S_1 to the potential energy $(E_2/c^2)\Phi$ of the radiation at S_2. But since the photon, as we now call Einstein's light quantum, has no rest mass, it is questionable whether the usual expression $m\Phi$ for the potential energy can legitimately be applied. This embarrassment can be avoided by reversion to the original attitude that the quantum principle is to be limited to the emission and absorption processes, and, thus, by focusing on the final states of the emitter S_2 and the absorber S_1 after the photon has been absorbed at S_1.[12] The change in energy of S_2 is $\Delta m_2c^2 + \Delta m_2\Phi$, where $\Delta m_2 = -(h\nu_2)/c^2$, while the change in energy at S_1 is $\Delta m_1c^2 + \Delta m_1\Phi$, where $\Delta m_1 = (h\nu_1)/c^2$. Setting the sum of these changes to zero, by conservation of energy, we have

$$\nu_1 = \nu_2[(1 + \Phi_2/c^2)/(1 + \Phi_1/c^2)]. \tag{1.5}$$

For $\Phi_1/c^2 \ll 1$, this relation is approximately

$$\nu_1 = \nu_2[1 + (\Phi_2 - \Phi_1)/c^2]. \tag{1.6}$$

This approach succeeds by treating the photon instrumentally and, thus, by sidestepping the problem of how to represent the energy of the photon itself in

[11]See A. Pais, 'Einstein on Particles, Fields and the Quantum Theory,' forthcoming in the *Proceedings of the Einstein Centennial Symposium*, Institute for Advanced Study, March 1979.

[12]The following argument is found in S. Weinberg, *Gravitation and Cosmology* (New York: John Wiley, 1972), pp. 84–85.

the gravitational field. That problem was not solved until almost a quarter of a century later by J. L. Synge: Synge's contribution will be discussed in Section 3 below, where we will see that only under restrictive conditions can the red shift be interpreted in terms of the change of the energy of the photon as it climbs through the gravitational field.

In 1912 Einstein tried once again to analyze uniformly accelerating motion and to characterize the state of a static gravitational field by means of the concept of a variable speed of light.[13] The attempt ended in a perplexing failure, for he was forced to conclude that his theory could consistently accommodate the equivalence principle only in infinitesimal regions. The difficulty derived from Einstein's proposed field equations, the details of which are not relevant here. What is important is that the failure of his 1912 theory seems to have convinced Einstein that he could no longer regard mathematics in its subtler forms 'as a pure luxury,'[14] and later that year he began in earnest to study the absolute differential calculus of Ricci and Levi-Civita. While Einstein had finally chosen the correct mathematical tool for building a relativistic theory of gravitation, he was over the next three years quite lost in the tensors; for he believed that a natural causality requirement precluded generally co-variant field equations. It was only at the end of 1915 that he succeeded in formulating the correct field equations.[15] After the completion of the general theory, Einstein continued to see, and encouraged others to see, the theory as embodying and as being a direct outcome of the principle of equivalence. The effects of such an attitude on the interpretation of the red shift will become evident in the following sections.

All of the heuristic derivations of the red shift can be faulted on various technical grounds. But to raise such objections is to miss the purpose of heuristic arguments, which is not to provide logically seamless proofs but rather to give a feel for the underlying physical mechanisms. It is precisely here that most of the heuristic red shift derivations fail — they are not good heuristics. For they are set in Newtonian or special relativistic space-time; but the red shift strongly suggests that gravitation cannot be adequately treated in a flat space-time.[16] Einstein's resort to the notions of a variable speed of light

[13] A. Einstein, 'Lichtgeschwindigkeit und Statik des Gravitationsfeldes,' *Annln. Phys.*, **38** (1912), 355 – 369, and 'Zur Theorie des statischen Gravitationsfeldes,' *ibid.*, 443 – 458. Einstein's theory is analyzed in our paper 'The Failure of the Principle of Equivalence in Einstein's 1912 Variable Speed of Light Theory,' forthcoming in the *Proceedings of the Memphis State University Einstein Centenary Conference*, March 1979.

[14] From a letter to Sommerfeld, dated October 29, 1912. See A. Hermann (ed.), *Albert Einstein/Arnold Sommerfeld Briefwechsel* (Basel: Schabe, 1968), p. 26.

[15] For details, see our paper 'Einstein and Hilbert: Two Months in the History of General Relativity,' *Archs. Hist. exact Sci.*, **19** (1978), 291 – 307, and 'Lost in the Tensors: Einstein's Struggles with Covariance Principles,' *Stud. Hist. Phil. Sci.*, **9** (1978), 251 – 278.

[16] See A. Schild, 'Gravitational Theories of the Whitehead Type,' *Evidence for Gravitational Theories*, C. Møller (ed.) (New York: Academic Press, 1962).

and variable clock rates in a gravitational field can be seen as an acknowledgement, albeit unconscious, of this point; but as we will now see, these notions served to obscure the role of the curvature of space-time as the light ray moves from source to receiver.

2. Attempts at a Formal Derivation of the Red Shift

In 1916 Einstein presented the first derivation of the red shift from his newly completed general theory.[17] Though couched in terms of the formalism of the new theory, the derivation actually relied on the same ideas as the 1907 and 1911 heuristic derivations, especially the idea that the rate of clocks is affected by the gravitational field. The gist of Einstein's construction is as follows. In a static gravitational field, the co-ordinate system can be chosen so that the line element has the form

$$ds^2 = -g_{\alpha\beta}dx_\alpha dx_\beta + g_{44}(dx^4)^2 \quad \alpha, \beta = 1, 2, 3, \tag{2.1}$$

where the metric potentials g_{ij} may depend upon the spatial co-ordinates x^α but are independent of the time co-ordinate x^4. (For the sake of simplicity we have set $c \equiv 1$.) For a 'unit clock' which is 'at rest' in the field, we have $ds = 1$ and $dx^1 = dx^2 = dx^3 = 0$. It follows, therefore, from (2.1) that

$$dx^4 = 1/\sqrt{g_{44}}. \tag{2.2}$$

For the case of a spherically symmetric gravitation field generated by a mass M, Einstein assumed that $g_{44} = (1 - CM/r)$, where C is a constant and r is the radial co-ordinate.[18] Einstein's conclusion is that in such a field, (2.2) entails that

> The clock goes more slowly if set up in the neighborhood of ponderable masses. From this it follows that the spectral lines of light reaching us from the surface of large stars must appear displaced towards the red end of the spectrum.[19]

To the modern eye, Einstein's derivation is no derivation at all, for the formula (2.2) expresses only a co-ordinate effect, and in contrast to the case of the bending of light, Einstein provided no deduction from the theory to explain what happens to a light ray or a photon as it passes through the gravitational field on its way from the Sun to the Earth.

[17] A. Einstein, 'Die Grundlage der allgemeinen Relativitätstheorie,' *Annln Phys.*, 49 (1916), 769–822.

[18] The 'Schwarzschild solution' of Einstein's field equations did not appear until later that year; see K. Schwarzschild, 'Über das Gravitationsfeld eines Massenpunktes nach der Einsteinschen Theorie,' *Sber. preuss. Akad. Wiss.*, 7 (1916), 189–196.

[19] Einstein, Ref. 17, p. 820; trans. from W. Perrett and G. B. Jeffrey.

Unfortunately, Einstein's 'derivation' was dressed up by the expositors of the general theory, and it quickly became codified in the literature as the official derivation. In the English-language literature the sequence was initiated by Willem de Sitter, who provided the first detailed review of Einstein's general theory to appear in English.[20] De Sitter transmitted his article to Arthur Stanley Eddington, then Secretary of the Royal Astronomical Society, along with a reprint of Einstein's 1916 'Grundlage' paper, apparently the only copy of this essay to reach England during the war years.[21] Eddington's subsequent rapid immersion in relativistic gravitation is evidenced by his numerous publications and lectures and his participation in the English eclipse expedition of 1919.[22] What is more important for our purposes, however, is Eddington's assumption of the role of chief expositor and defender of Einstein's theory in England; the quasi-official recognition of this role can readily be confirmed by turning the pages of *Nature* and *Observatory*, where letters raising objections to the theory were often followed by a response from Eddington. Thus, it is hardly surprising that Eddington's version of the red shift derivation, a version which closely parallels de Sitter's, was reproduced in most the English-language text books of the period.[23]

Eddington's treatment, as it appears in his *Report*, starts with the premise that an atom 'is a natural clock which ought to give an invariant measure of an interval δs; that is to say, the interval δs corresponding to one vibration of the atom is always the same'.[24] So if $(dx^4)_1$ and $(dx^4)_2$ are the periods of two similar atoms 'at rest' in the field at points 1 and 2 respectively, the application of (2.1) and the relation $(ds)_1 = (ds)_2$ (the Premise) gives

$$\sqrt{g_{44}}\,]_1 (dx^4)_1 = \sqrt{g_{44}}\,]_2 (dx^4)_2. \tag{2.3}$$

If 1 refers to the Sun and 2 to the Earth, then since $\sqrt{g_{44}}\,]_1 < \sqrt{g_{44}}\,]_2$ we have $(dx^4)_1 > (dx^4)_2$ so that 'the solar atom thus vibrates more slowly, and its spectral lines will be displaced towards the red'.[25]

[20] W. de Sitter, 'On Einstein's Theory of Gravitation and Its Astronomical Consequences,' *Mon. Not. R. Astr. Soc.*, 76 (1916), 699 – 728, and 77 (1917), 155 – 184.

[21] See 'Arthur Stanley Eddington,' *Obit. Not. Fell. R. Soc. Lond.*, 5 (1945), 113 – 125. Prof. S. Chandrasekhar has informed us that in conversation Eddington confirmed these details. We are most grateful to Prof. Chandrasekhar for his illuminating comments on this period.

[22] In a letter to Einstein, dated December 1, 1919, Eddington wrote: 'I have been kept busy lecturing and writing on your theory. My Report on Relativity is sold out and is being reprinted . . . I had a huge audience at the Cambridge Philosophical Society a few days ago, and hundreds were turned away unable to get near the room.' (Einstein Papers, Princeton University, IBI microfilm Reel No. 9.)

[23] See, for example, E. Cunningham, *Relativity, the Electron Theory, and Gravitation* (London: Longmans, Green, 1921), pp. 121 – 122; and L. Silberstein, *The Theory of Relativity and Gravitation* (New York: Van Nostrand, 1922), pp. 102 – 105.

[24] A. S. Eddington, *Report on the Relativity Theory of Gravitation* (London: Fleetwood Press), 1st edn. 1918, 2nd edn. 1920; quotation from p. 56 of the 2nd edn.

[25] Eddington, Ref. 24, 2nd edn., p. 57.

This treatment is exactly backwards from what is wanted; namely, the co-ordinate time intervals equal and the proper time intervals different. Henceforth we will refer to it as the 'backwards derivation'. The confusion lies in Eddington's misapplication of the Premise. Grant, for the moment, the Premise. What needs to be compared in the first instance are not the proper time intervals $(ds)_1$ and $(ds)_2$ corresponding respectively to the vibrations of similar atoms to points 1 and 2; rather, the most immediate task is to compare the proper time interval $(ds)_1$ for a vibration at 1 with the proper time interval $[ds]_2$ between the reception of two light signals sent from 1 as markers of the beginning and end of the vibration (see Fig. 2).

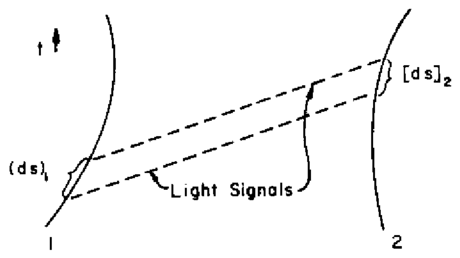

Fig. 2.

In a stationary co-ordinate system the co-ordinate time interval for a vibration is transmitted without change from 1 to 2 so that $(dx^4)_1$ for the emission at 1 is the same as $[dx^4]_2$ for the reception at 2. In effect, this was demonstrated in 1920 by Max von Laue for the more restrictive case of a static gravitational field;[26] he proved that in such a case, Maxwell's equations admit solutions in which the time co-ordinate enters in the form $\exp(i\nu x^4)$, where the co-ordinate frequency ν is a constant, independent of x^4 and x^α.[27] For a source and receiver 'at rest', (2.1) then gives

$$(ds)_1/[ds]_2 = \{\sqrt{g_{44}}]_1(dx^4)_1\}/\{\overline{g_{44}}]_2[dx_4]_2\}$$

$$= \sqrt{g_{44}}]_1/\sqrt{g_{44}}]_2. \tag{2.4}$$

or in terms of the proper frequency, which is inversely proportional to the proper time interval,

$$\nu_2/\nu_1 = \sqrt{g_{44}}]_1/\sqrt{g_{44}}]_2. \tag{2.5}$$

[26]See the Appendix for definitions of stationary and static.

[27]M. von Laue, 'Theoretisches über neuere optische Beobachtungen zur Relativitätstheorie,' *Phys. Z.*, **21** (1920), 659–662, and **22** (1921), 332.

Now is the place for the Premise to be applied, at the end and not at the beginning of the argument: since the proper frequency ν_1 of the atom at 1 is the same as the proper frequency of a similar atom at 2, the observer at 2 will see a shift towards the red in the spectral lines coming from atoms at 1, as measured relative to the frequency of similar atoms at 2.

In order to avoid ambiguity, double subscripts on the frequency could be used, the first subscript denoting the source and the second the point of measurement. Thus, (2.5) would read

$$\nu_{1,2}/\nu_{1,1} = \sqrt{g_{44}}\rvert_1/\sqrt{g_{44}}\rvert_2. \tag{2.5$'$}$$

In this notation, the Premise reads $\nu_{1,1} = \nu_{2,2}$, and the red shift effect is then

$$\nu_{1,2}/\nu_{2,2} = \sqrt{g_{44}}\rvert_1/\sqrt{g_{44}}\rvert_2. \tag{2.5$''$}$$

If the reader thinks that this notation is overly fussy and pedantic, he need only read on.

The first and, as far as we have been able to determine, the only explicit query of the backwards derivation to appear in the literature of the period was posed by James Rice in a letter to *Nature*.[28] Rice was puzzled, as well he should have been, as to why Einstein's theory predicted any shift in the spectral lines if, as seems implicit in the backwards derivation, the proper time interval ds and not the co-ordinate interval dx^4 is transmitted unchanged. Eddington's reply pinpointed the essential fact:

> The rule deduced from Einstein's theory for comparing the passage of two light pulses at the points A and A' respectively is not $ds = ds'$, but $dt = dt'$, *provided that the coordinates used are such that the velocity of light does not change with* t.[29]

But Eddington managed to leave the reader with a confused impression:

> At a point in the laboratory (r = constant), dt_1 for a light vibration from a solar atom differs from dt_2 for a terrestrial atom. It follows from the formula (A) [the formula for the line element] that ds_1 and ds_2 will differ in the same ratio since we are now concerned only with the relation of dt and ds on earth. The intermediary quantity t is thus eliminated; and the difference in the light received from solar and terrestrial sources is an absolute one, which it is hoped the spectroscope will detect.[30]

The elimination of the 'intermediate quantity' t (our x^4) is indeed the crucial aspect of the argument leading to (2.5); but the elimination is not achieved, as Eddington seems to indicate, by considering dt_1 for a solar atom and dt_2 for a terrestrial atom. Further, we are told that ds_1 and ds_2 differ in the same ratio as

[28] J. Rice, 'The Predicted Shift of the Fraunhofer Lines,' *Nature, Lond.* **104** (1920), 598.
[29] A. S. Eddington, *Nature, Lond.* **104** (1920), 598; the italics are Eddington's.
[30] Eddington, Ref. 29, p. 599.

dt_1 and dt_2; but we are not told what the ratio is — the upside ratio of the backwards derivation or the correct ratio of (2.4) — nor for that matter is it made clear just what ds_1 and ds_2 now denote. And nowhere does Eddington explicitly state the correct formula for the red shift. A similar unclear impression is left by Eddington's remarks at a meeting of the Royal Society of London, held on the day his response to Rice was published in *Nature*.[31]

Two months later in a response to Guillaume,[32] Eddington swept away some of the misimpressions and re-emphasized the essential point:

> It is perhaps unfortunate that in the best known discussions of this problem the question of what happens along the light-wave from Sun to Earth, through the non-Euclidean space – time, is scantily treated. From an absolute point of view, it is here that the cause of the spectral shift occurs, since ds changes continuously along the path (dt remaining constant). This completion of the argument is one of those things which are obvious if you happen to approach them in the right way; but it causes a great deal of difficulty if you have not grasped the full significance of the fact that the co-ordinate system has been so chosen that the velocity of light at any point does not involve the co-ordinate t.[33]

One wonders whether Eddington was indulging in some mild self-criticism since the 'best known discussions of this problem' were Eddington's own and since 'a great deal of difficulty' has resulted from his not approaching the problem in the right way. If so, he did not take the criticism to heart, for the 'completion of the argument' was something which Eddington never fully carried out. All of the editions of his widely read and admired *Space, Time, and Gravitation*[34] contain a treatment of the red shift much closer to the backwards derivation than to the correct one. This is perhaps attributable to the semi-popular nature of that work and to the understandable desire not to burden the general reader with too much detail. But then one would expect that the highly technical *Mathematical Theory of Relativity* would provide Eddington the proper setting for putting the matter straight once and for all. One is disappointed, however. There, Eddington emphasized that ds 'becomes gradually modified as the waves take their course through non-Euclidean space – time' and that this modification is the source of the red shift.[35] But still the promise to complete the argument is not fulfilled, and Eddington continued to refer to dt as the 'time' of vibration of an atom, and

[31] A. S. Eddington, in 'Discussion on the Theory of Relativity,' *Proc. R. Soc.*, **97** (1920), 72 – 74.

[32] E. Guillaume, 'Displacement of the Solar Lines and the Einstein Effect,' *Observatory*, **43** (1920), 227 – 228.

[33] A. S. Eddington, *Observatory*, **43** (1920), 229. In a nonstationary gravitational field the time coordinate t cannot be chosen so that dt is preserved; consequently, the derivation of the red-shift formula is more complicated in such a case (see section 6 below).

[34] A. S. Eddington, *Space, Time and Gravitation* (Cambridge: Cambridge University Press), 1st edn. 1920, reprinted 1921, 1923, 1929, etc.

[35] A. S. Eddington, *The Mathematical Theory of Relativity* (Cambridge: Cambridge University Press), 1st edn. 1923, 2nd edn. 1924; quotation from p. 92 of the 2nd edn.

from the fact that the line element assumed the form $ds^2 = \gamma dt^2$ for an atom 'at rest', he inferred that 'the times of vibration of similar atoms will be inversely proportional to $\sqrt{\gamma}$.'[36]

As an ironic and sad footnote, it should be added that Rice, whose letter to *Nature* called attention to the emperor's new clothes, published his own textbook three years later; in it he repeated the backwards derivation.[37]

The miasma which hung over the theoretical treatment of the red shift even affected Hermann Weyl's otherwise elegant *Raum – Zeit – Materie.* In all five editions,[38] the red shift was approached within the confines of a static gravitational field, using the relation

$$ds = f dt \tag{2.7}$$

between the proper time ds and the 'cosmic' (co-ordinate) time t at a fixed point of space.[39] It is assumed that

> If two sodium atoms at rest are objectively fully alike, then the events that give rise to light-waves of the D-line in each must have the same frequency, as measured in *proper time.*[40]

The conclusion drawn in the first three editions is that if f has the values f_1 and f_2 respectively at the locations 1 and 2 and if τ_1 and τ_2 are respectively the frequencies, as measured in 'cosmic time', of the atoms at 1 and 2, then there will exist the relationship

$$f_1\tau_1 = f_2\tau_2; \quad \tau_1/\tau_2 = f_2/f_1. \tag{2.8}$$

Weyl's commentary on this formula is as follows:

> ...the light waves emitted by an atom will have, of course, the same frequency, measured in *cosmic* time, at all points of space. Consequently, if we compare the sodium D-line produced in a spectroscope by the light sent from a star of great mass with the same line sent by an earth-source into the same spectroscope, there should be a slight displacement of the former line towards the red as compared with the latter.[41]

In the fourth edition, two crucial changes are made. First, the symbol ν is substituted for the previously used τ; and second, the formula (2.8) is inverted to read[42]

[36] Eddington, Ref. 35, p. 92.

[37] J. Rice, *Relativity: A Systematic Treatment of Einstein's Theory* (London: Longman, Green, 1923), pp. 287 – 289.

[38] H. Weyl, *Raum-Zeit-Materie* (Berlin: Julius Springer), 1st edn. 1918, 2nd edn. 1918, 3rd edn. 1919, 4th edn. 1921, 5th edn. 1923.

[39] In our notation, $t = x^4$ and $f = \sqrt{g_{44}}$.

[40] Weyl, Ref. 38, 3rd edn., p. 211; the italics are Weyl's.

[41] Weyl, Ref. 38, 3rd edn., p. 212; the italics are Weyl's.

[42] Weyl, Ref. 38, 4th edn., p. 223.

$$\nu_1/f_1 = \nu_2/f_2. \tag{2.9}$$

What accounts for the apparent sleight-of-hand involved in these changes? The textual evidence is not sufficient to provide a firm answer, but it invites the following speculation. Instinctively, Weyl must have seen that (2.8) is the right form for the red shift, but at the same time he must have sensed that (2.8) was not justified by the considerations he adduced — perhaps this accounts for the use of the symbol τ for frequency in spite of the fact that τ is more standardly used to denote period rather than frequency. And in addition, he would have wanted, if only unconsciously, his formula for the red shift to agree with the then current backwards formula. In any case, Weyl's corrected argument, as given in the 4th and 5th edns, seems to be this. Since the proper time periods of the two atoms are the same, we have from (2.7)

$$f_1 dt_1 = f_2 dt_2. \tag{2.10}$$

And since frequency is inversely proportional to period, (2.9) follows. At first glance, this seems to be just the old backwards derivation, but in Weyl's version, there is a difference; namely, he is explicit that ν in (2.9) is 'cosmic' (co-ordinate) frequency and that in a static field the cosmic frequency of the light wave emitted by the atom at 1 does not change in transmission to 2. Hence, in (2.9) ν_1 can be taken to be the cosmic frequency, as measured at 2, of the light wave emitted at 1. But since we are now referring everything to a single point — location 2—the ratios of the co-ordinate frequencies are the same as the ratios of the proper frequencies (since the f factor cancels out), and so (2.9) should hold for proper frequencies as well. The resulting formula is beautifully ambiguous: it seems to agree with everyone else's upside down ratio and to contradict the correct ratio (2.5); but on the other hand, ν_1 and ν_2 can be interpreted respectively as the $\nu_{1,2}$ and $\nu_{2,2}$ of the pedantic notation, turning his (2.9) into the correct relation (2.5″).

One is tempted to postulate a kind of demonic possession or mass hysteria in order to explain how some of the acutest minds of 20th-century physics, in possession of all the facts needed to arrive at a correct conclusion, could uniformly produce an incorrect or ambiguous result. In the case of von Laue, the temptation is almost irresistible. In Vol. 2 of his *Relativitätstheorie*, von Laue follows the route leading to (2.4). But obviously wanting his final result to agree with everyone else's (backwards) formula, he concludes that since 'the number of oscillations serves directly as the measure of proper time',[43] we obtain

$$\nu_1/\nu_2 = \sqrt{g_{44}}\big]_1/\sqrt{g_{44}}\big]_2. \tag{2.11}$$

[43]M. von Laue, *Die Relativitätstheorie* (Braunschweig: F. Vieweg, 1921), Vol. 2, p. 188.

The upshot, of course, is that the spectral rays of the Sun come to us with a frequency less than that of similar terrestrial sources. This, says von Laue, is what is called the displacement towards the red. The ambiguity which allows the backwards formula to pass muster is resolved by the more pedantic notation. Von Laue's conclusion $\nu_{\text{Sun}} < \nu_{\text{Earth}}$, which seems, *prima facie,* to express a displacement towards the red, is really $\nu_{\text{Sun, Sun}} < \nu_{\text{Sun, Earth}}$; and since $\nu_{\text{Sun, Sun}} = \nu_{\text{Earth, Earth}}$, the real upshot is that $\nu_{\text{Earth, Earth}} < \nu_{\text{Sun, Earth}}$—a blue shift!

In summary, the leading theoretical physicists of the period, while grasping the essential ideas, still did not manage to produce a clean and unambiguous formal derivation of the spectral shift; the lesser lights tended to get the ideas as well as the formula wrong.

3. The Photon Frequency Definition of the Spectral Shift

In 1918 Eddington noted that if the displacement of the Fraunhofer lines of the solar spectrum were confirmed, "it would be the first *experimental* evidence that relativity holds for quantum phenomena."[44] Contained in this remark are two challenges. The quantum theory associates a frequency ν with a quantum E of energy, where the ratio $h \equiv E/\nu$ is assumed to be a universal constant. Thus, if the solar spectral shift is conceived in terms of a light quantum which is emitted from the Sun with frequency ν_1 and energy E_1 and which is received on the Earth with frequency ν_2 and energy E_2, then the shift can be expressed in the form

$$\nu_2/\nu_1 = E_2/E_1. \tag{3.1}$$

But in the context of general relativity, (3.1) remains a meaningless formula until the expression E is interpreted in terms of the space – time geometry — this is the first challenge. Once it is met, it remains to be proved that the interpretation is consistent with both the quantum theory and general relativity; that is, it must be shown that the ratio E_2/E_1 coincides with the ratio $(ds)_1/[ds]_2$ of the previous section. Meeting these challenges would not only have helped to clarify the confused situation surrounding the theoretical derivation of the red shift, but it would have also demonstrated a communality to the theories of quanta and gravitation and would have provided just the sort of consistency check on both theories needed during their early stages of development.

That these challenges were not immediately taken up is hardly surprising in view of the fact that the resistence to Einstein's light quantum hypothesis continued until Compton's experiments of 1923. What is curious is that the

[44]Eddington, Ref. 24, 2nd edn., p. 58; the italics are Eddington's.

challenges seem not to have been formulated in the literature until the 1930s. Although these developments take us outside of the main period with which we are concerned in this paper, they are sufficiently important for a clarification of the issues discussed above to justify a digression.

The initial development came in 1933 in a paper by Kermack *et al.*,[45] but their general treatment was not entirely satisfactory, principally because it used a wave interpretation of the photon and because it did not link the energy of the photon with changes in the proper mass of the emitting and receiving mechanisms. Both defects were overcome in an article published two years later, in 1935, by J. L. Synge.[46] Synge assumed that the world line of a photon is a null geodesic in general relativistic space – time and that the photon has an energy – momentum vector P tangent to and parallelly propagated along its world line. A system of observers is realized geometrically in terms of a field V of unit timelike vectors, the vector at any point giving the instantaneous state of motion of the observer at that point. The energy of the photon, as measured by a given observer, is then defined by

$$E \equiv P \cdot V. \tag{3.2}$$

By viewing the history of the photon in terms of a thin two-dimensional ribbon of null lines, Synge was able to establish that (3.2) satisfies the consistency demand $\nu_2/\nu_1 = E_2/E_1 = (ds)_1/[ds]_2$.

The implications of (3.2) for the more familiar co-ordinate expressions of the red shift are easy to draw out for the case of a stationary frame. The invariant characterization of the stationarity of a frame V is that V is proportional to a Killing vector field X; that is, $V = hX$, where h is a positive function and X satisfies Killing's equations

$$X_{(i;j)} = 0. \tag{3.3}$$

Using (3.3) and the fact that the path of the photon is a null geodesic, it follows readily (see Appendix) that on the path of the photon

$$P \cdot X = \text{constant}. \tag{3.4}$$

Thus, the energy or frequency ratio reduces to

[45] W. O. Kermach, W. H. M'Crea and E. T. Whittaker, 'On the Properties of Null Geodesics and Their Applications to the Theory of Radiation,' *Proc. R. Phys. Soc. Edinb.* **53** (1933), 31 – 47.

[46] J. L. Synge, 'The Proportionality of Energy and Frequency for a Photon in General Relativity,' *Q. Jl. Math.*, **6** (1935), 199 – 204.

$$E_2/E_1 = P \cdot V|_2/P \cdot V|_1 \\ = h|_2/h|_1. \tag{3.5}$$

In a co-ordinate system chosen so that $X = (0, 0, 0, 1)$ Killing's equations give the familiar co-ordinate expression of stationarity — $g_{ij,4} = 0$. And the normalization condition on V reads $V \cdot V = 1 = h^2 X \cdot X = h^2 g_{44}$. Thus, (3.5) becomes

$$E_2/E_1 = \sqrt{g_{44}}\rceil_1/\sqrt{g_{44}}\rceil_2. \tag{3.6}$$

So stationarity alone, without the additional assumption of staticity, suffices for the classical expression of the red shift.

In the Newtonian approximation where we can set $g_{44} = (1 + 2\Phi)$ with Φ as the Newtonian potential, (3.6) gives, to the first order approximation in small Φ,

$$(E_2 - E_1)/E_1 = (\Phi_1 - \Phi_2). \tag{3.7}$$

And the change in the frequency can be interpreted classically in terms of the change in the 'potential energy of the photon in the gravitational field'. It should be emphasized, however, that this interpretation is valid only for very special situations while the definition (3.2) applies to an arbitrary gravitational field.

It is also worth noting that the standard co-ordinate expressions, (2.5) and (3.6), are inapplicable in many cosmological models; for example, most of the 'expanding universe' solutions to Einstein's field equations do not possess any timelike Killing fields. And even in models that do admit stationary frames, the relevant astronomical objects may not be co-moving with such a frame. An example of the red shift in a non-stationary frame will be given later in section 6.

4. The Experimental Background

The history of the solar red shift measurements has been described in detail by Forbes,[47] and our review will, therefore, be brief. In the latter 19th century the only known cause of spectral displacement was the Doppler principle: light from a source moving away from the receiver is shifted towards the red with respect to light from a source of the same kind at rest with respect to the receiver, while light from a source moving towards the receiver is shifted

[47] E. Forbes, 'A History of the Solar Red Shift Problem,' *Ann. Sci.*, 17 (1961), 129 – 164.

towards the blue. Rowland[48] conducted an extensive survey of the Fraunhofer spectrum in the 1880s and discovered that solar lines appear displaced, most often towards the red, as compared with electric arc lines. Jewell[49] repeated the measurements and established that the amount of the displacement varied with the element considered, the line considered, and even with the same line when measured on different photographic plates. He argued that these effects were not due to experimental error alone and that they could not be Doppler effects either, since the latter should be directly proportional to the wavelength, which the solar shifts were not, and should not depend on the line intensity, which the solar shifts appeared to do.

Almost simultaneously with these developments, other research came upon another phenomenon which seemed to offer an explanation. Mohler and Humpheries[50] found in 1896 that arc spectra depend on pressure; for a certain range of pressures, they established that the spectral lines are displaced towards the red in proportion to the increase in pressure and that the displacement varies from line to line. Qualitatively then, pressure displacement looked like solar displacements and it was natural to suspect that the latter are a pressure effect, and Jewell *et al.* collaborated in proposing this explanation.[51] Developments after the turn of the century tended to undercut this account. Fabry and Buisson demonstrated that electric arc spectra broaden asymmetrically under pressure, besides shifting to the red.[52] The fact that no such asymmetrical broadening was observed in the solar spectra made it unlikely that the solar red shift could be due to pressure alone. Moreover, the general appearance of the solar absorption lines was similar to the arc spectrum produced in a vacuum, again suggesting that pressure was not the central factor. Finally, using the pressure hypothesis, Jewell had inferred that in the outer solar layer where the lines are produced, the pressure is 2 – 7 atm; but Evershed and others produced evidence that the pressure in the outer layers is but a single atmosphere.[53]

A further feature of electric arc spectra, called the 'pole effect,' depends on the distance between the electrodes, the current, and the material from which the arc is produced. Because of the lack of standardization in the arc construction and the pressure at which the arc was operated, the conflicting

[48]H. Rowland, 'Preliminary Table of Solar-Spectrum Wave Lenghts,' (Carnegie Institute of Washington, 1895).

[49]L. Jewell, 'The Coincidence of Solar and Metallic Lines,' *Astrophys. J.*, 3 (1896), 89 – 113.

[50]J. Mohler and W. Humpheries, 'Effect of Pressure on the Wave-Lengths of Lines in the Arc-Spectra of Certain Elements,' *Astrophys. J.*, 3 (1896), 114 – 137.

[51]L. Jewell, J. Mohler and W. Humpheries, 'Note on the Pressure of the "Reversing Layer" of the Solar Atmosphere,' *Astrophys. J.*, 3 (1896), 138 – 140.

[52]H. Buisson and C. Fabry, 'Mesures de Petites Variations de Longueurs d'Onde par la Méthode Interférentielle,' *J. Phys. théor. appl.*, 9 (1910), 298 – 316.

[53]J. Evershed, 'Pressure in the Reversing Layer,' *Bull. Kodaikánal Obs.*, **18** (1909); and 'A New Interpretation of the General Displacement of the Lines of the Solar Spectrum Towards the Red,' *ibid.*, **36** (1913).

results of different observers were hard to compare. Thus, an important discovery in this area was that the cyanogen lines are relatively free of both the pressure and pole effects, and consequently a good deal of the debate over Einstein's red shift prediction turned on the interpretation of the CN spectrum.

After the turn of the century, attempts were made to obtain quantitative information about the variations of the solar spectrum from different regions of the sun. Halm[54] found a gradual increase in wavelengths for two FeI lines from the center to the limb of the Sun, with wavelengths at the nearest limb exceeding those at the center by 12mA, and other workers found similar increases for other species. By 1910 it had been established that the center-limb shifts were in direct proportion to the wavelength. The discovery that the CN bands also showed a center-limb shift gave evidence that the effect was due, at least in part to a Doppler effect created by radial convection currents. According to this view, which was championed by Evershed, the red shift at the Sun's limb should be approximately zero, since the radial convection movements will be orthogonal to the line of sight. Evershed and Royds[55] and then St. John,[56] obtained limb arc shifts that were too large to agree with this hypothesis. In response to these negative results, Evershed proposed a curious explanation according to which the Earth exerts a special repulsion on the Sun, leading him to believe that light from the part of the Sun invisible from the Earth would have a significantly different spectrum from that of direct sunlight. Observations of Venus did not lend any support to this suggestion,[57] but Evershed continued to advance it for some time.

We have examined above Einstein's 1911 argument for a gravitational red shift; in that paper, Einstein explicitly proposed that the claim might be tested by examination of the solar spectra, and he referred to the work of Fabry and Buisson. In the years immediately following, although Einstein himself was distinguished, his attempts at constructing a relativistic theory of gravitation were not, and there is little evidence of a rush by observers to put his proposal to the test. The first practical astronomer to give detailed attention to Einstein's ideas seems to have been Erwin Freundlich, then a young astronomer at the Berlin observatory who had been in correspondence with Einstein since 1911 regarding various means of testing Einstein's predictions.[58]

[54]J. Halm, "Über eine bisher unbekannte Verschiebung der Fraunhoferschen Linien des Sonnenspektrums,' *Astr. Nachr.*, **173** (1907), 272 – 287.

[55]J. Evershed and T. Royds, 'On the Displacements of the Spectrum Lines at the Sun's Limb,' *Bull. Kodaikánal Obs.*, **39**, (1914).

[56]C. E. St. John, 'The Principle of Generalized Relativity and the Displacement of the Fraunhofer Lines Towards the Red,' *Astrophys. J.*, **46** (1917), 249 – 265.

[57]See J. Evershed, 'The Displacement of the Solar Lines Reflected by Venus,' *Observatory*, **42** (1919), 51 – 52, and C. E. St. John and S. B. Nicholson, 'On Systematic Displacements of Lines in Spectra of Venus,' *Astrophys. J.*, **53** (1921), 380 – 391.

[58]In the Einstein Papers at Princeton University there is a letter from Einstein to Freundlich, dated September 1, 1911, responding to a letter of Freundlich's (not in the file) about Einstein's

In 1914 Freundlich published a paper comparing the red shift data from Fabry and Buisson, Evershed and Royds, and from St. John with both Einstein's and Nordström's theories.[59] At about the same time, Schwarzschild[60] reported measurements of Sun-arc displacements at different positions on the solar disk, with the aim of confirming or disconfirming Einstein's prediction. The results were all smaller than the Einstein hypothesis required, but still close enough to seem promising. The following year Freundlich gave a statistical argument for the gravitational red shift using data from stellar red shifts, stellar masses, and stellar motions,[61] but the argument was criticized by Seeliger,[62] and the case for or against the 'Einstein effect' was generally understood to turn on the solar red shifts.

Between 1915 and 1919 numerous measurements were made, chiefly by Evershed and St. John, with the aim of testing Einstein's claim. The results were persistently negative. Thus, St. John, reporting a study of the pressure-free CN lines in both the center and the limb of the Sun concluded that 'within the limits of error the measurements show no evidence of an effect of the order deduced from the equivalence relativity principle.'[63] Evershed obtained values for red shifts which were higher than St. John's but much below what Einstein required. Moreover, because of the superior equipment available to St. John at the Mt. Wilson Observatory, his measurements were given a greater weight. By 1919, the general opinion seems to have been that the outlook was glum indeed for the gravitational red shift, and, as we will describe in the following section, much of the theoretical discussion was premised on the assumption that the facts were not in accord with Einstein's theory.

1919 saw new developments in the debate over the 'Einstein effect'. Grebe and Bachem,[64] working in Bonn, has measured shifts of CN lines and obtained results that were about 80% of those required by the theory. In 1920 they obtained results even more favorable to the theory, having with Einstein's aid

1911 paper on the influence of gravitation on the propagation of light (IBI, microfilm reel No. 11). Two facts stand out from the frequent correspondence which followed: Einstein's interest in the details of Freundlich's experiments and Einstein's gratitude to Freundlich for his efforts to confirm Einstein's developing gravitational theories.

[59]E. Freundlich, 'Über die Verschiebung der Sonnenlinien nach dem roten Ende auf Grund der Hypothesen von Einstein und Nordström,' *Phys. Z.*, **15** (1914), 369–371.

[60]K. Schwarzschild, 'Über die Verschiebung der Bande bei 3.883Å in Sonnenspektrum,' *Sber. preuss. Akad. Wiss.*, **47** (1914), 1201–1213.

[61]E. Freundlich, 'Über die Gravitationsverschiebung der Spektrallinien bei Fixsternen,' *Astr. Nachr.*, **202** (1915), 16–23.

[62]H. Seeliger, 'Über die Gravitationswirkung auf die Spektralinien,' *Astr. Nachr.*, **202** (1916), 83–86.

[63]C. E. St. John, 'A Search for an Einstein Relativity Gravitational Effect in the Sun,' *Proc. nat. Acad. Sci. U.S.A.*, **3** (1917), 450–452.

[64]L. Grebe and A. Bachem, 'Über den Einstein Effekt im Gravitationsfeld der Sonne,' *Verh. dt. Phys. Ges.*, **21** (1919), 454–464.

obtained a microphotometer from Freundlich.[65] Further, they offered explanations as to why other observers, and they themselves, had obtained results in disagreement with Einstein's theory. Grebe and Bachem argued that observed values lower than those predicted by Einstein were due to misidentification of terrestrial emission lines with solar absorption lines; unsymmetrical emission lines, they first suggested, could correspond in reality to symmetrical absorption lines. Later they argued that because nearby lines could merge on the photographic plate, the apparent pattern of intensities and locations of peaks could be quite different from the actual ones, leading to a misidentification of lines. Only those lines which were well-isolated from metallic lines could be trusted. Their argument for the 'Einstein effect' was, in consequence, based on only nine CN lines.

While Einstein was enthusiastic about Grebe and Bachem's work, physicists of a more experimental bent were generally not, and the criticisms were quick in coming. W. G. Duffield added an addendum to his survey article on the displacement of spectral lines in which he discounted Grebe and Bachem's results. Duffield doubted both their values and their explanation of others' failures to obtain Einstein's value: 'In any case,' he concluded, 'the interpretation cannot be as simple as that given by Grebe and Bachem.'[66] An extract of a letter of Einstein's praising Grebe and Bachem's work, was published in *Nature*, and it elicited a devastating critique by St. John. 'Two young physicists in Bonn have now securely demonstrated the red shift of the spectral lines in the Sun and have explained the basis of earlier failures' Einstein wrote.[67] St. John attacked both their measurements and their explanation.[68] The dispersion of their spectrograph, St. John argued, was but 1mm/Å, too low for a region of the spectrum in which the Fraunhofer lines are so dense. Light was directed into the spectrograph from a heliostat with no evidence given that the orientation of the images made the slit and the solar axis parellel, which lack would introduce additional difficulties in measuring lines close to one another. The solar image used was but 5 cm in diameter, which meant that 'the most extreme care in guiding would be necessary to render negligible the effect of the Sun's rotation, and there seems to have been no provision for accurate guiding'; this last error alone, St. John maintained, could easily account for a third of the observed shifts. Finally, the comparison spectrum was made half before and half after the solar spectrum, rather than simultaneously, and St. John claimed that this procedure could introduce large spurious displacements. Rather similar criticisms were published in German by

[65]L. Grebe and A. Bachem, 'Über die Einsteinverschiebung im Gravitationsfeld der Sonne,' *Z. Phys.*, **1** (1920), 51–54; 'Die Einsteinsche Gravitationsverschiebung in Sonnenspektrum der Sticktoffbande λ = 3883AE,' *ibid.*, **2** (1920), 415–422.

[66]W. G. Duffield, 'The Displacements of the Spectrum Lines and the Equivalence Hypothesis,' *Mon. Not. R. Astr. Soc.*, **80** (1920), 262–272.

[67]*Nature, Lond.* **104** (1920), 565.

[68]C. E. St. John, 'The Displacement of Solar Spectral Lines,' *Observatory*, **43** (1920), 260–262.

L. Glaser,[69] but, unlike St. John's criticisms, in a tone that was altogether contemptuous both of Grebe and Bachem's work and Einstein's theory.

Starting about 1920, it became almost the fashion to obtain experimental results that agreed with Einstein's prediction. Grebe[70] argued that when St. John's correction for the center-edge difference in the red shift was applied, old measurements due to Rowland, Uhler, and Patterson gave Einstein's value for the red shift. Perot[71] published new measurements in agreement with the theory; and Fabry and Buisson[72] reviewed their old measurements, done before Einstein's prediction, and concluded that they were in accord with the theory. It seems likely that this newfound eagerness to obtain experimental confirmation of the red shift was in large part brought about by the announcement of the results of the 1919 English eclipse expedition. But even in the early 1920s, Evershed and St. John did not obtain red shifts conforming to Einstein's requirements. By 1921, however, Evershed[73] had reluctantly concluded that the weight of the *prima facie* evidence was in favor of the theoretical prediction. St. John maintained the contrary view, and in the symposium on relativity published in *Nature* in 1921 he argued that the observations were in conflict with the theory.[74] But even St. John was no longer convinced that Einstein was wrong, and by the late 1920s he had been won over to Einstein's view.

In retrospect, the eclipse measurements may loom larger in the confirmation of Einstein's red shift formula than the red shift measurements themselves. There were always measurements of *some* lines that gave values close to Einstein's; from Fabry and Buisson, through Schwarzschild to Grebe and Bachem that remains so. The question was at least as much one of arguments as of measurements: *if* one was willing to argue that the close measurements were really evidence that the theoretical prediction was exact, and *if* one was willing to argue that the measurements that were not even close should be thrown out altogether, then the Einstein prediction could be seen to be confirmed by the red shift observations. Grebe and Bachem were willing to make the arguments — their observations may have been of some help, but were not essential, for most of the same arguments could have been made from earlier data. After the eclipse results were known, other observers were also willing to make the same kinds of arguments.

[69]L. C. Glaser, 'Über die Gravitationsverschiebung der Fraunhoferschen Linien,' *Phys. Z.*, **23** (1922), 100 – 102.

[70]L. Grebe, 'Sonnengravitation und Rotverschiebung,' *Z. Phys.*, **4** (1921), 105 – 109.

[71]A. Perot, 'Comparaison des Longueurs d'Onde d'une Raie de Bande du Cyanogène dans la Lumière du Soleil et dans celle d'une Source Terrestre,' *C. R. hebd. Séanc. 171* (1920), 229 – 232.

[72]H. Buisson and C. Fabry, 'Sur le Déplacement des Raies Solaires sous l'Action du Champ de Gravitation,' *Acad. Sci. Paris* **172** (1921), 1020 – 1022.

[73]J. Evershed, 'Relativity Shift in the Solar Spectrum,' *Observatory*, **44** (1921), 243 – 245.

[74]C. E. St. John, Ref. 1.

5. The Theorists' Reactions to the Experimental Results

Judgments of the bearing of the red shift results on Einstein's general theory varied from theorist to theorist and depended on several factors: on how seriously one took the apparently negative results, on whether or not the red shift was thought to be a genuine consequence of the theory, on whether one thought that if the red shift prediction proved false the entire theory must be abandoned. The last issue, in turn, depended on a number of others which we will discuss below. Although when the red shift was thought not to agree with Einstein's prediction, this fact was generally thought to be of the first importance, later, when it was widely believed that Einstein's prediction had been borne out, theorists tended to regard the red shift as a relatively unimportant confirmation of the theory. Einstein's prediction, if wrong, was strong negative evidence, if right, weak positive evidence, as though to confound the philosophers of science.

Einstein himself was eternally optimistic about the red shift. Thus in May 1915 he wrote to Walter Dallenbach that 'One of the two important experimental consequences has already been brilliantly confirmed, namely the shift of spectral lines due to the gravitational potential.'[75] Einstein probably had in mind Schwarzschild's work, and perhaps also Freundlich's early work. The 'brilliant confirmation' was, as we have seen, dimmed by a flood of negative results. When Grebe and Bachem's work appeared, Einstein was again convinced that the red shift prediction has been confirmed, and he wrote to Eddington early in 1920[76] with an enthusiastic summary of their work, even sending Eddington a copy of one of Grebe and Bachem's papers. In other correspondence he continued to place great reliance on Grebe and Bachem's work. For his own part, Eddington[77] held the rather curious view that red shift values other than those required by the Einstein effect were not evidence against Einstein's theory, so long as the observed red shift was not absolutely zero. His reason seems to have been that as long as there is non-zero red shift there is evidence of something causing a shift in spectra, and we do not know how

[75]Letter from Einstein to W. Dallenbach, May 31, 1915: Einstein Papers, Princeton University, IBI, Microfilm reel No. 9. We are grateful to Dr. Otto Nathan, Trustee of the Albert Einstein Estate, for permission to publish the excerpts from Einstein's correspondence used in this paper. On November 28, 1915, Einstein wrote to Sommerfeld: 'Only the intrigues of wretched people are preventing this last important proof [*i.e.* the deflection of light] of the theory from being carried out. However, this is not really so painful to me, because the theory, especially if one also considers the quantitative confirmation of the spectral line shifts, appears to me to be sufficiently secure.' The letter is published in A. Hermann (ed.), *Albert Einstein/Arnold Sommerfeld Briefwechsel* (Basel: Schabe, 1968), p. 36.

[76]Letter from Einstein to Eddington, dated February 2, 1920; Einstein Papers, Princeton University, IBI, microfilm reel No. 9. Eddington's response, dated March 15, 1920, shows that he was not convinced: 'Thank you very much for the paper by Grebe and Bachem. The results are very interesting; and look convincing although I am scarcely qualified to judge. I hear that St. John has been making further researches, with magnesium and other lines, still getting zero results; so I expect that for some time to come spectroscopists will be divided as to what the result really is.'

[77]See Eddington, Ref. 34.

other causes combine with the Einstein effect to produce a total red shift. Until about 1923, and perhaps even later, the majority of theorists seem to have thought that red shift measurements were against Einstein's prediction. Not all of them, however, concluded that the red shift provided evidence against the theory.

Prior to 1921, a significant number of theorists doubted that Einstein's general theory did require a gravitational spectral shift; in some cases the doubts stemmed from uncertainties in the handling of Einstein's principle of equivalence and the attendant notion that the rate of clocks is affected by the gravitational field, while in other cases the doubts centered on attempted formal deductions from the postulates of the theory. Larmor and Cunningham belonged to the former category, as can be seen from an article Cunningham wrote for *Nature* a month after the dramatic announcement of the success of the English eclipse expedition:

> Sir Joseph Lamor . . . is of the opinion that Einstein's theory does not in reality predict the [red-shift] displacement at all. The present writer shares his opinion. Imagine, in fact, two identical atoms originally at a great distance from both Sun and Earth. They have the same period. Let an observer A accompany one of these into the gravitational field of the Sun, and an observer B accompany the other into the field of the Earth. In consequence of A and B having moved into different gravitational fields, they make different changes in their scales of time, so that actually the solar observer A will find a different period for the solar atom from that which B, on the Earth, attribute to his atom.[78]

When others tried to duplicate Einstein's brilliant heuristic juggling act, the result was often just useless confusion. One example of the latter group was Leigh Page, who argued against the red shift in terms of his treatment of the deflection of high speed particles by a gravitational field.[79]

By the end of 1921, however, most of the informed theorists were convinced that Einstein's theory did indeed entail a spectral shift, though as we have seen, no one had provided a completely non-problematic derivation and many were unclear about the physical basis of the 'Einstein effect'. When the differences of opinion regarding the observations themselves are added to this picture, it is hardly surprising that we find differing assessments of the implications of the experimental evidence. Any attempt to encompass this diversity within a neat classification scheme is bound to blur some distinctions; but some principle of organization is needed, and ours will be the simple one

[78]E. Cunningham, 'Einstein's Relativity Theory of Gravitation,' *Nature, Lond.*, **104** (1919), 395. See also J. Larmor, 'Gravitation and Light,' *Nature, Lond.*, **104** (1919), 412. Cunningham also argued that the principle of equivalence does not necessarily entail a red shift; see A.C.D. Crommelin, 'The Theory of Relativity,' *Nature, Lond.*, **104** (1920), 631 – 632, and E. Cunningham, 'Discussion on the Theory of Relativity,' *Proc. R. Soc.*, 97 (1920), 77.

[79]L. Page, 'Gravitational Deflection of High Speed Particles,' *Nature, Lond.*, **104** (1920), 692 – 693.

of dividing people into two categories: on the one hand, those who thought that if the negative experimental evidence persisted, Einstein's theory would have to be abandoned and, on the other hand, those who thought that the theory could be protected against the negative results by some modification or by the addition of some auxiliary hypothesis. These categories have very fuzzy boundaries, and their occupants tended to shift back and forth as time progressed.

Ludwig Silberstein was the most vocal of those who saw the negative red shift results as *prima facie* falsifiers of general relativity. He took Einstein's general theory to stand on two foundational legs — the principle of general co-variance and the principle of equivalence — and in order to jettison the red shift prediction Silberstein tried to construct a theory which would stand without the support of the principle of equivalence.[80] In Silberstein's mind, the negative red shift results even threatened to undermine the support given by the detection of the bending of light:

> . . . it seems premature to interpret this result [the bending of light] as a verification of Einstein's theory, not merely in view of the small outstanding discrepancies, but chiefly in view of the failure of detecting the spectrum shift predicted by the theory, with which the theory stands or falls.[81]

And he went on to suggest that the deflection of light by the Sun can be viewed as a reason to resurrect the very aether which Einstein's special theory had seemed to bury.

As far as the logic of confirmation went, Einstein was in complete agreement with Silberstein; in fact, in support of his interpretation, Silberstein quoted a remark of Einstein's which had appeared in the *Times* of London for November 28, 1919: 'If any deduction from it [the general theory] proves untenable, it must be given up. A modification of it seems impossible without destruction of the whole.'[82] In a similar vein, Einstein wrote to Eddington on December 15, 1919:

> According to my persuasion, the red-shift of the spectral lines is an absolutely compelling consequence of relativity theory. If it should prove that this effect does not exist in nature, then the entire theory would have to be abandoned.[83]

Einstein's holistic attitude towards the general theory was in marked contrast to his own attitude towards his earlier theory of relativity. In his first attempts to extend relativity to gravitation, he had found that his ideas about

[80] L. Silberstein, 'General Relativity without the Equivalence Hypothesis,' *Phil. Mag.*, **36** (1918), 94 – 128.

[81] L. Silberstein, 'The Recent Eclipse Results and the Stokes – Planck Aether,' *Phil. Mag.*, **39** (1920), 161 – 170.

[82] Quoted in L. Silberstein, in the 'Discussion on the Theory of Relativity,' *Mon. Not. R. Astr. Soc.*, **80** (1919), 113.

[83] Einstein Papers, Princeton University, IBI, microfilm reel No. 9.

the equivalence of reference frames conflicted with the constancy of the velocity of light, and this with Lorentz invariance. In response to criticisms by Max Abraham and others, he had taken pains to argue that the principle of relativity and the constancy of the velocity of light were separable assumptions, and although tied together in the special theory, one could be given up without surrendering the other.[84] Now his attitude was quite different, apparently for several reasons. In the first place, the general theory had proved to have a much greater logical unity than even Einstein had expected while devising it. The field equations had not merely the virtue of a kind of simplicity and elegance; although Einstein had not realized it before 1916,[85] the field equations logically entail the conservation laws, and this result was generally understood by those familiar with the theory. Moreover, by 1919 it was already being claimed that the field equations even logically entail the equations of motion.[86] In the second place, Einstein took the principle of equivalence to be the touchstone of general relativity and he believed the red shift to be a direct consequence of this principle. Finally, Einstein may very well have been willing to assert that if the red shift prediction were false then the entire theory must be abandoned exactly because he never doubted that the antecedent of the conditional was false. To Besso he wrote on May 28, 1921: 'The displacement of the spectral lines towards the red has now been verified . . . at no moment did I doubt the fact that it would turn out this way.'[87]

Eddington's initial reading of the confirmational situation echoed Einstein's; in his *Report*, he wrote that

> the displacement of the Fraunhofer lines is a necessary and fundamental condition for the acceptance of Einstein's theory; and . . . if it is really non-existent . . . we should have to reject the whole theory constructed on the principle of equivalence.[88]

Even here, however, Eddington wanted to leave room for maneuvering:

> Possibly a compromise might be effected by supposing that gravitation is an attribute only of matter in bulk but not of individual atoms; but this would involve a fundamental restatement of the whole theory.[89]

Later Eddington was to offer other compromises.

An urge to find a compromise which would protect Einstein's theory

[84]See A. Einstein, 'Relativität und Gravitation. Erwiederung auf eine Besmerkung von M. Abraham,' *Annln. Phys.*, **38** (1912), 1059 – 1064.

[85]On Einstein's ignorance of this relationship before 1916, see our paper 'Einstein and Hilbert: Two Months in the History of General Relativity,' *Archs. Hist. exact Sci.*, **19** (1979), 291 – 307.

[86]See D. Moyer, 'Revolution in Science, 1919,' pre-print 1979.

[87]P. Speziali (ed.), *Albert Einstein – Michele Besso Correspondence* (Paris: Hermann, 1972), pp. 163 – 164.

[88]Eddington, Ref. 24, 2nd edn., p. 58.

[89]Eddington, Ref. 24, 2nd edn., p. 58.

characterized by far the largest group of English physicists. The urge is perhaps explained by the combination of two facts. On one hand, the success of the English eclipse expedition and the subsequent pride in scientific objectivity trimphant over the nationalistic prejudices of the War years gave the English a feeling that they had a stake in the theory.[90] On the other hand, they were not as confident of the theory as Einstein was; nor did they share his view that his theory was a logical unity, immune to tampering and adjustment. The result was a number of attempts to tamper and adjust.

The most radical tampering was proposed by James Jeans in the form of an attack on the metrical interpretation of the line element:

> What, however, is meant by *ds*? The simplest interpretation is to regard it merely as a conventional algebraic symbol defined by (*D*) [the formula for the line element]. Then the law of motion predicts Mercury's orbit and light deflection, but it does not, I think, make any prediction about the shift in the Fraunhofer lines. Einstein gives to *ds* a special interpretation in terms of space and time; for him *ds* is a line element in a distorted space – time continuum. The hypothesis from which this special interpretation is derived requires inevitably, Einstein considers, a shift in the Fraunhofer lines toward the red, and I do not think that we can dispute that he is right. It also requires us to believe in an objective curved four-dimensional space, and grave difficulties are disclosed by a consideration of this space . . . The reality of the four-dimensional continuum is, I think, beyond dispute, but the reality of the twists and kinks in it do not appear to be. Decisive knowledge as to the shift of the Fraunhofer lines would go far towards settling this question.[91]

Jeans' assessment was half-right. A shift in the Fraunhofer lines is required by a space – time continuum distorted by 'twists and kinks'. But his proposal to drop the metrical interpretation of *ds* in case of no red shift is one that would save the theory only by emasculating it. Not only would the proposal turn the theory into a mere formal computing device, but it would leave unanswered the question of how the computational outputs for the orbit of Mercury and the path of a light ray passing by the Sun are to be connected with observations. Eddington put the point well in a passage from *Space, Time, and Gravitation* that is obviously aimed at Jeans, although he is not mentioned by name:

> Without some geometrical interpretation of s our conclusions as to the courses of planets and light-waves cannot be connected with the astronomical measurements which verify them. The track of a light-wave in terms of the co-ordinates r, θ, t cannot be tested directly; the co-ordinates afford only a temporary resting place; and

[90] In closing the December 1919 meeting of the Royal Astronomical Society, the President said: '. . . we may take a reasonable pride in the contribution which our Society has made to the development of this subject through its representatives on the Eclipse Committee, and we may well hope there will be general satisfaction in the knowledge that our national prejudice did not prevent us from doing anything that we could to forward the progress of science.' *Mon. Not. R. Astr. Soc.*, **43** (1919), 118.

[91] J. Jeans, 'Discussion on the Theory of Relativity,' *Proc. R. Soc.*, **97** (1920), 71 – 72.

> the measurement of the displacement of the star-image on the photographic plate involves a reconversion from co-ordinates to *s*, which here appears in its significance as the interval in clock-scale geometry.[92]

Although he rejected Jeans' compromise, Eddington explored other modes of escape. The most desperate one started with the possibility that the events marking the beginning and end of an atomic vibration are not 'absolute events' to which a definite proper time interval corresponds.

> . . . if . . . an atomic vibration is determined by the revolution of an electron around a nucleus, it is not marked by definite events. A revolution means the return to the same position as before; but we cannot define what is the same position as before without reference to some mesh-system [co-ordinate system]. Hence it is not clear that there is any absolute interval corresponding to the vibration of an atom; an absolute interval only exists between events absolutely defined.[93]

A return to the same position is not, contrary to Eddington, a co-ordinate dependent notion, though it is a reference frame-dependent one. But this is just as it should be since the red shift itself is a frame-dependent effect. That Eddington should even suggest this mode of escape shows how far he was from an understanding of the role of co-ordinates and reference frames in general relativity.[94]

Eddington's other suggestion — that atoms on the Sun do not act as 'natural clocks' — was the most popular mode of escape on either side of the English channel; in addition to Eddington, Cunningham, Silberstein, Jeffreys, and de Sitter took it up at one time or another.[95] The most picturesque version of this suggestion surmised that just as jarring a grandfather clock may alter its time keeping properties, so the hammering of the solar atoms upon one another may alter their proper frequencies. The weakness of this version is that what is needed to explain a null red shift is a systematic alteration of the solar atoms that still leaves the observed line structure in the solar spectrum. Eddington admits that it is only 'just possible' that some yet-to-be-specified systematic difference in the behavior of solar and terrestrial atoms would provide the needed alteration.[96]

Another and more subtle version surmised that solar and terrestrial atoms

[92]Eddington, Ref. 34, p. 127.

[93]Eddington, Ref. 34, p. 132.

[94]A similar misunderstanding is contained in Eddington's reply to Guillaume: 'To discuss what is the absolute time duration of a vibration without reference to any co-ordinate system is like discussing what is the absolute distance from Cambridge without reference to any point that it is distant from.' *Observatory*, **43** (1920), 228.

[95]See E. Cunningham, *Relativity, the Electron Theory, and Gravitation* (London: Longmans, Green, 1921), p. 122; L. Silberstein, *The Theory of Relativity and Gravitation* (New York: Van Nostrand, 1922), pp. 104 – 105; H. Jeffreys, 'On the Crucial Tests of Einstein's Theory of Gravitation,' *Mon. Not. R. Astro. Soc.*, **80** (1919), 138 – 154; W. de Sitter, Letter to Einstein dated December 1, 1919, Einstein Papers, Princeton University, IBI, microfilm reel No. 20.

[96]Eddington, Ref. 34, p. 132.

do not vibrate with the same proper frequencies because they react to differences in the curvature of the regions they occupy. Eddington quickly dashed this hope by estimating that the effect of the curvature would be of the order M^2/r^2 whereas the order of magnitude M/r is needed to account for a null red shift.[97]

Despite the lack of any plausible way to implement the suggestion that solar atoms do not serve as 'natural clocks', this idea remained the most favored mode of escape. What makes the discussion of the question strange is that in the absence of decisive experimental evidence, all need not be given over to speculation. An important *gedanken* consistency test can be performed: take a periodic system (a 'clock') and apply the equations of motion of general relativity to it to determine what the theory implies about the time keeping properties in various gravitational fields. In 1920 Einstein did remark that it is a 'logical weakness' of the then-current version of the theory that rods and clocks must be taken as primitives instead of being constructed as solutions of differential equations.[98] The weakness lay not in the theory itself but in the ingenuity of the theorists; and it is surprising that not until a quarter of a century later, when Møller[99] studied the conditions under which general relativity predicts that a harmonic oscillator clock measures proper time, was the weakness overcome.

Finally, we report Fokker's suggestion, made in a letter to Einstein, that the null red shift results could be explained by a 'compensation of the gravitational shift by an electrical-Weylian effect'.[100] It is not clear what Fokker had in mind, or even that he had a definite proposal — he closed the letter by saying that he 'had not yet found the inspiration' to work out his idea. Unfortunately, either Einstein did not reply, or else his reply has been lost; but his general attitude about the relevance for the red shift of Weyl's attempt to unify gravitation and electromagnetism[101] is formulated in a letter to Besso written in 1920:

> Weyl's theory is not able to help here [with the red shift]. Either it gives the independence of rods and clocks from their histories, in which case it is of no use; or it does supply that dependence, in which case it is false because of the sharpness of atomic rays and frequencies.[102]

Einstein had already endorsed the second horn of this dilemma. He argued that

[97]Eddington, Ref. 34, p. 132.

[98]Einstein's remarks were made in the discussion that followed von Laue's paper, Ref. 27, p. 662.

[99]C. Møller, 'Old Problems in the General Theory of Relativity Viewed from a New Angle,' *K. danske Vidensk. Selsk. Matematisk-fysiske Meddelelser,* **30** (1955), (10).

[100]A. D. Fokker, letter to Einstein dated July 26, 1919: Einstein Papers, IBI, microfilm reel No. 10.

[101]See H. Weyl, 'Gravitation und Elekritizität,' *Sber. preuss Akad. Wiss.,* **25** (1918), 465 – 480.

[102]Ref. 87, p. 152.

Weyl's theory implies the dependency of atomic frequencies on the electromagnetic potentials they have encountered and that such a dependence contradicts the existence of well-defined atomic spectra.[103] Of course, it is possible that systematic differences between the histories of solar and terrestrial atoms could produce, via a Weylian mechanism, definite but different spectra for the two groups of atoms and that the differences cancel out the gravitational red shift — this may have been what Fokker had in mind. But Eddington's dismissive label of 'just possible' seems appropriate here as it was in the previous case, for in both instances a surprising coincidence is needed to produce just the right compensation.

None of the modes of escape reported here stands up to impartial scrutiny. But the fact that so many suggestions for escape were offered by so many people shows how swiftly Einstein's general theory had won a concerned following. The following greatly increased after the success of the eclipse observations, but it was already considerable before the success was reported.

6. The Confirmation Issue

The red shift, so controversial when it was thought to conflict with Einstein's theory, has been judged by later commentators to be of little worth as evidence *for* the theory. It is frequently claimed that the solar red shift provides no evidence at all for the field equations of general relativity. The grounds most commonly given are that the red shift can be deduced from principles other than the field equations; indeed, it is very often claimed that the fact that the red shift follows from the equivalence principle shows that the red shift does not confirm the field equations.[104] This very common opinion is also very curious. For even putting aside the vagueness of the equivalence principle, the claim seems to be that a phenomenon does not confirm an hypothesis if that phenomenon can be deduced from some weaker hypothesis entailed by the first, or from some logically independent hypothesis. Such a principle is untenable: each of the classical tests of general relativity can be deduced from premises that are logically weaker than the field equations; the advance of Mercury's perihelion and the bending of light rays near the Sun can be deduced respectively from the equations of motion in a Schwarzschild field and from the Schwarzschild line element, both logically weaker than the field equations.

There is nothing inconsistent in the supposition that a prediction, if false, is strong evidence against a theory but if true is only weak evidence in its favor.

[103]Einstein wrote to Besso: 'From the beginning, I have been generally convinced of the falsity of Weyl's theory,' Ref. 87, p. 152.

[104]See, for example, H. Robertson and T. Noonan, *Relativity and Cosmology* (Philadelphia: W. B. Saunders, 1968), p. 241; and R. Adler, M. Bazin and M. Schiffer, *Introduction to General Relativity* (New York: McGraw-Hill, 1975), pp. 137-139.

Nonetheless, the argument that the red shift is of little value as a positive test of the theory *because* it follows from the equivalence principle was scarcely ever made between 1915 and the early twenties. Starting with a paper of Harold Jeffreys' published in 1919, however, a different line of argument led to a similar conclusion. Jeffreys' paper, 'On the Crucial Test of Einstein's Theory of Gravitation'[105] scarcely mentions the red shift; the paper is concerned with what the advance of the perihelion of Mercury and the deflection of light near massive bodies determine about the field equation, given a number of auxiliary assumptions (these include, *e.g.* that the space-time metric is of Lorentz signature, that material particles follow geodesics of the metric, that light follows null geodesics of the metric, that the field of the Sun is to good approximation spherically symmetric). Jeffreys argues that the two phenomena together with such auxiliary assumptions virtually determine the field equations. The evident but unstated implication is that the red shift result, even if it were as Einstein requires, would of itself (in combination with the same auxiliaries) be insufficient to determine the field equations, and in combination with the other two phenomena would place no restriction on the field equations not obtainable from the perihelion advance and gravitational deflection alone.

An approach very much like Jeffreys' was hinted at in the first edition of Eddington's *Mathematical Theory of Relativity,* but it was not presented explicitly until the second edition. We follow the presentation of J. L. Anderson.[106]

In terms of local spherical co-ordinates, the most general static and spherically symmetric line element, assumed to represent the exterior gravitational field of a non-rotating stationary mass M, can be written in the form

$$ds^2 = -e^\lambda dr^2 - r^2 d\theta^2 - r^2\sin^2\theta d\phi^2 - e^\mu (dx^4)^2 \tag{6.1}$$

where λ and μ may be functions of the radical co-ordinate r but are independent of the time co-ordinate x^4. Let us assume that the factors e^λ and e^μ can be expanded in power series as

$$\begin{aligned} e^\mu &= 1 + \alpha_1(2m/r) + \alpha_2(2m/r)^2 + \ldots \\ e^\lambda &= 1 + \beta_1(2m/r) + \beta_2(2m/r)^2 + \ldots\ldots \end{aligned} \tag{6.2}$$

Correspondence with Newtonian theory in the lowest order of m/r fixes m =

[105] H. Jeffreys, Ref. 95.

[106] J. L. Anderson, *Principles of Relativity Physics* (New York: Academic Press, 1967), Ch. 12, See also L. I. Schiff, 'Comparison of Theory and Observation,' *Relativity Theory and Astrophysics,* J. Ehlers (ed.) (Providence: American Mathematical Society, 1967).

GM/c^2, where G is the gravitational constant, and $\alpha_1 = -1$. Eddington found that for the resulting line element the advance $\delta\omega$ per revolution in the perihelion of a planet of negligible mass in an orbit of eccentricity ε and semi-major axis a is given by

$$\delta\omega = \frac{2\pi m}{a(1 - \varepsilon^2)} (2 + \beta_1 - 2\alpha_2). \tag{6.3}$$

Also, the angle of deflection θ of light, to the lowest order in m/r, is

$$\theta = \frac{2m}{R} (1 + \beta_1) \tag{6.4}$$

where R is the radius of closest approach. Agreement with the results of the 1919 eclipse measurements is obtained by setting $\beta_1 = +1$. When this value is inserted into (6.3), agreement with the observed value for the perihelion of Mercury is obtained by setting $\alpha_2 = 0$. These are just the values required by Einstein's field equations (*i.e.* the Schwarzschild solution).

Neglecting terms in the expansions of e^λ and e^μ which are not discriminable by measurement, we see that the measurement of the deflection of light permits us to determine e^λ and the measurement of light deflection in conjunction with the measurement of the advance of the perihelion permits us to determine both e^λ and e^μ. Moreover, the precision of these measurements is such that they determine e^λ and e^μ more precisely than do the hypotheses — that the metric field is static and spherically symmetric, that light and matter follow geodesics of the field, and that in lowest order the trajectories reduce to the Newtonian ones — which must be used in computing e^λ and e^μ from θ and $\delta\omega$. The computed e^λ and e^μ enable us to determine all the observationally discriminable parameters in the Einstein field equations when the latter are given the appropriate co-ordinate expression.

By contrast, to first order in m/r the red shift for *all* the metrics of the class (6.1) which have the right Newtonian limit is

$$(\nu_{2,2} - \nu_{1,2})/\nu_{2,2} = m(\frac{1}{r_1} - \frac{1}{r_2}). \tag{6.5}$$

Thus, the first order solar spectral shift tells us nothing more about space-time than is already given in the hypotheses; it does test the theory, but it tests nothing more, and nothing more accurately, than is already done by the fact that Newton's equations are valid to first order.

Furthermore, it seems that even if the spectral shifts were measured to arbitrarily great accuracy, they would only serve to constrain the μ parameter but would give no information about λ since the red shift formula involves the former but not the latter. The force of this second complaint against the in-principle confirmatory powers of the spectral shifts depends in part on the implicit restriction to a case where both source and receiver are at rest in a stationary frame. But in typical cases, either the receiver or source or both are in geodesic motion, and a frame cannot be both stationary and geodesic if there is to be a non-null spectral shift (see Appendix). In what follows, we will analyze the solar spectral shift for a receiver in geodesic motion in the metric (6.1). A similar analysis also applies to the case where the Earth is regarded as the source of the gravitational field and an Earth-orbiting satellite acts as the source of the light signals.

Tolman[107] has computed the connexion coefficients for (6.1). Substituting these into the geodesic equations for a test body whose motion is initially in the plane $\theta = \pi/2$ gives

$$\frac{d^2t}{ds^2} + \frac{d\mu}{ds}\frac{dt}{ds} = 0. \tag{6.6}$$

The first integral

$$dt/ds = Ke^{-\mu},\ K = \text{constant} \tag{6.7}$$

gives the relation between the proper time and the co-ordinate time on a geodesic. Setting $ds^2 = 0$ in (6.1) gives the co-ordinate velocity of light

$$\left(\frac{dr}{dt}\right)^2 = e^{\mu-\lambda} - r^2\left(\frac{d\theta}{dt}\right)^2 - r^2\sin^2\theta\left(\frac{d\phi}{dt}\right)^2. \tag{6.8}$$

For a light signal moving radially, this reduces to

$$\frac{dr}{dt} = \pm e^{(\mu-\lambda)/2}. \tag{6.9}$$

Equation (6.9) can be used to express the time of arrival t_2 at r_2 of a light signal

[107]R. C. Tolman, *Relativity, Thermodynamics and Cosmology* (Oxford: Clarendon Press, 1934), p. 241.

directed radially outward as a function of the time t_1 is emission at the surface of the Sun r_1:

$$t_2 = t_1 + \int_{r_1}^{r_2} e^{(\mu-\lambda)/2} dr. \tag{6.10}$$

Differentiating (6.10) yields

$$\delta t_2 = \left[1 + e^{[\mu(r_2) - \lambda(r_2)]/2} \frac{dr_2}{dt}\right] \delta t_1 \tag{6.11}$$

since $dr_1/dt = 0$. Computing $(ds)_1$ from (6.1) for a source at rest in the stationary co-ordinates and $[ds]_2$ from (6.7) for a receiver in geodesic motion gives for the red shift

$$\begin{aligned} \nu_{2,2}/\nu_{1,2} &= [ds]_2/(ds)_1 \\ &= \left\{\frac{e^{\mu(r_2)}}{K} \delta t_2\right\} / \left\{e^{(\mu(r_1)/2)} \delta t_1\right\} \\ &= \frac{1}{K} \left\{e^{\mu(r_2) - (\mu(r_1)/2)}\right\} \left\{1 + e^{[\mu(r_2) - \lambda(r_2)]/2} \frac{dr_2}{dt}\right\}. \end{aligned} \tag{6.12}$$

When the radial motion of the receiver is zero, (6.12) reduces to

$$\nu_{2,2}/\nu_{1,2} = \frac{1}{K} e^{\mu(r_2) - (\mu(r_1)/2)} \tag{6.13}$$

which should be compared with the standard red shift formula for a receiver that remains at rest in the stationary frame rather than performing geodesic motion:

$$\nu_{2,2}/\nu_{1,2} = e^{[\mu(r_2) - \mu(r_1)]/2}. \tag{6.14}$$

For the Sun – Earth shift, the difference between (6.13) and (6.14) makes for no practical difference in the observations. From (6.12) and (6.13) we see that the factor λ can be delimited by a combination of sufficiently accurate measurements of the spectral shift and a knowledge of the radial velocity of the receiver. Thus, the ability of the spectral shift measurements to probe the space-time metric is not subject to inherent limitations but is bounded only by the accuracy of the experiments. The contrary opinion is the result of an artificial restriction to stationary frames.

7. The Interpretation of the Spectral Shifts

Issues concerning the interpretation of the spectral shifts come in two forms. First, there are attempts to understand the effect in terms of pre-general relativistic concepts. Such attempts show that while on one level Einstein's general theory won wide acceptance, on another level there was a withholding of full confidence. A second and more interesting set of issues concerns the status of the spectral shifts within the general theory. Foremost among these issues is the continuing debate as to whether the shifts should be seen as a Doppler or as a gravitational effect. The first detailed brief for the Doppler interpretation was filed by Lanczos[108] in 1923. Text books of the 1920s and 1930s often discussed the red shift under the Doppler label.[109] And the Doppler interpretation continues to receive distinguished support.[110] Since any account of the nature and status of the spectral shifts would be incomplete without some discussion of this matter, we will briefly review the relevant considerations. Our starting point is Synge's warning that the debate can degenerate into 'windy warfare' unless there is careful attention to the meanings of the terms employed.[111] We will argue that even when Synge's warning is heeded, 'windy warfare' may still be an accurate description of the debate because the Doppler vs gravitational dichotomy is too crude to do justice to the complexities encountered in general relativity.

Consider first the non-Doppler interpretation. The argument for this view is seemingly straightforward: for source and receiver at rest in a frame where the relative distances between the reference points do not change, a spectral shift cannot be a Doppler effect and must, therefore, be attributed to the action of gravitation. The trick is to find a suitable means to assure that the relative distances are constant in time. One popular method is to use radar measurements: if the round trip proper time for light signal to travel from the reference point P_1 to another point P_2 and back again does not change with time, then the radar distance from P_1 to P_2 is said to be constant. This method singles out stationary frames as the setting for the gravitational interpretation, for it can be shown that a frame is stationary if and only if the radar distance

[108]C. Lanczos, 'Über die Rotverschiebung in der Sitterschen Welt,' *Z. Phys.*, **17** (1923), 168–189.

[109]See, for example, R. C. Tolman, Ref. 107.

[110]See J. L. Synge, *Relativity: The General Theory* (Amsterdam: North-Holland, 1976). Synge says: 'In attributing a *cause* to the spectral shift, one would say . . . that the spectral shift was caused by the relative velocity of source and receiver; it is in fact a Doppler effect in the original sense of the term,' (p. 123).

[111]Synge, Ref. 110, p. 123.

between any pair of reference points is constant in time (see Appendix). Unfortunately, these settings are not always congenial to the gravitational interpretation. In Minkowski space – time consider a rigid frame rotating with constant angular velocity relative to an inertial frame ('rotating disk'). This frame is stationary, and as Einstein pointed out, observers at rest at points lying at different distances from the axis of rotation will detect a spectral shift. But the effect obviously cannot be legitimately interpreted as a gravitational effect since there is no real gravitational field present (flat space – time).[112]

The advocate of the gravitational interpretation can overcome this embarrassment by retreating to the more restrictive case of static frames, *i.e.* frames that are non-rotating as well as stationary. Nor is the restriction *ad hoc*, for it permits a more intuitive way of measuring relative distances. A non-rotating frame V is hypersurface orthogonal, and, thus, there is a natural simultaneity associated with V. At any 'instant', the spatial distance between two observers can be computed by measuring the distance in the corresponding spacelike hypersurface between their instantaneous positions.[113] For observers at rest in the static frame, the relative distances in this sense are, of course, unchanging. Alas, even in this more restrictive setting the consistency of the gravitational interpretation is open to challenge. Let x, y, z, t be inertial co-ordinates in Minkowski space – time, and in the region where $x > 0$ and $x^2 > t^2$ define the vector field X whose components are $X^i = (t, 0, 0, x)$. It is easy to verify that X is a timelike, nonrotating, Killing vector.[114] Thus, $V \equiv hX$, $h = (X \cdot X)^{-1/2}$, is a static frame. But since the normalization factor h is not constant, there will be a spectral shift for source and receiver at rest in the frame. Again, however, the effect is not a gravitational one since no real gravitational field is present. This example, and the preceding one also, can be ruled out by the imposition of global requirements on frames because the frames in question cannot be defined on all of Minkowski space – time; but such a move has an *ad hoc* flavor since the red shift is normally discussed in terms of local conditions.

The additional requirement that the frame be non-accelerating is a local condition which will rule out the troublesome example of the preceding paragraph, but only at disastrous expense. For in any space – time, flat or not, a frame V which is both geodesic and stationary is one in which there is no spectral shift for source and receiver co-moving with the frame (see Appendix).

The spectral shift predicted by general relativity theory is a complicated function of the states of motion of the source and receiver and the curvature of

[112] We are indebted to Robert Geroch for bringing this example to our attention.

[113] The space – time metric g_{ij} induces on the spacelike hypersurface a positive definite metric $h_{ij} \equiv g_{ij} + V_i V_j$ (see Appendix); spatial distances are measured in h_{ij}.

[114] This example was supplied by David Malament.

the space – time between them, and the task of separating these variables is a difficult if not impossible one. The use of stationary and static frames might have been thought to quash the effects of motion, but as the above examples show, this is clearly not so since the spectral shift in these cases can have no other cause. On the other hand, the motion is not of the sort that happily lends itself to a Doppler interpretation.

Unless the frame is co-variantly constant (*i.e.* $V^i_{;j} = 0$), the world lines of the reference points can be considered as being non-parallel to one another , and thus, the reference points can be regarded as being in relative motion. Taking advantage of this fact, Synge[115] has defined a concept of relative velocity between the source and receiver of a light signal: the four-velocity vector of the source at the instant of emission is parallel transported along the light ray to the receiver; the resulting vector is projected onto the hyperplane orthogonal to the four velocity of the receiver at the point of reception; and the projection is then normalized to give the relative velocity.[116] Synge was able to show that the theoretical value of the spectral shift can be expressed directly in terms of this quantity. However, Synge's relative velocity may bear only a distant resemblance to the velocity concepts used in classical and special relativistic expressions for the Doppler shift; for instance, it may be non-zero for source and receiver at rest in a static frame where, as we saw above, there is a natural sense in which the spatial distances do not change. Moreover, Synge's inference from the fact that the Riemann tensor does not appear in his Doppler-like formula for the spectral shift to the conclusion that the effect is not a gravitational one can be misleading. For although the Riemann tensor may not make an explicit appearance, the curvature can make itself felt in Synge's formula through its effects on the parallel transport of vectors.

Further investigations may pinpoint various classes of cases where the Doppler or gravitational labels can be happily applied. But the above considerations are enough to show that in general it is wise to speak of the spectral shift without attaching these labels — otherwise one runs the risk of being guilty of windy warfare.

8. Conclusion

In recent years the red shift has typically been treated, both by physicists and historians, as a distinctly minor issue in the development of gravitational theory. Our view is rather different: the red shift is a litmus, and its coloring

[115]Synge, Ref. 110, pp. 119 ff.

[116]If there is more than one null geodesic connecting the points of emission and reception, then Synge's concept of relative velocity must be relativized to a null path if parallel transport along different null geodesics leads to different results.

reveals most of the major themes that dominated the development avD reception of general relativity. Einstein's early derivations of the red shift show his most characteristic style of work — heuristic, allusive, sometimes baffling, but unfailingly fruitful. His derivation of the red shift from within general relativity shows something of the same characteristics, but it reveals rather more directly the difficulty Einstein had in treating the relations between physical quantities and co-ordinate expressions in a coherent way. In this, as we have seen, Einstein was joined by many of the best mathematical physicists at work on gravitation at the time. In this regard, the red shift is only an extreme case of a difficulty that was common enough for other crucial tests as well. One can find all manner of confusion about the perihelion advance and the bending of light, but for some reason, hard to put one's finger on, the confusions about these tests were not nearly so widespread as were those concerning the red shift. These very confusions and misunderstandings about how to apply the theory, misunderstandings shared by many of its advocates, doubtless helped to make the scientific debate over general relativity a little more furious and chaotic. Anti-relativists did not have to create their own misunderstandings of the theory (although many of them did not hesitate); the misunderstandings were already there to be enjoyed and used in the battle.

More than a litmus for the historians, the red shift debates illustrate, besides, how much more intricate and delicate issues of confirmation can be and were than the representations of physicists and philosophers sometimes lead us to believe. Historically, we find neither clear-cut agreement nor disagreement between measurements and theory, but instead a dispersion of results which could be interpreted either in favor of the theory or against it, according to one's determination. We find, besides, a kind of psychological dependence of the red shift on the eclipse results, since it was only after the 1919 eclipse expedition that a number of solar scientists found the will to interpret their results in favor of the 'Einstein effect'. And we find more power for testing general relativity than is usually attributed to the red shift.

Altogether, there may be no other single topic which so vividly illustrates the intellectual ferment, the styles of work, the profundity and the confusion, associated with general theory of relativity.

Appendix

Notation

Throughout the paper we use modern notation — *e.g.* ordinary and co-variant derivatives are denoted respectively by comma and semi-colon and the standard conventions that go with it, *e.g.* the Einstein summation convention on repeated indices. Round and square brackets denote respectively symmetrization and anti-symmetrization.

Definitions

Since reference frames are crucial to the derivation and interpretation of the red shift, it will be useful to review some of the basic concepts. Let (M, g) be a *relativistic space-time*, where M is a four-dimensional differentiable manifold and g is pseudo-Riemannian metric of signature $(---+)$. A *reference frame* for (M, g) is a unit timelike vector field V on M, *i.e.* $V \cdot V \equiv g(V, V) = 1$. The frame V is *stationary* iff it is proportional to a Killing vector field; that is, $V = hX$ where h is a positive function and X is a vector field satisfying $\pounds_X g = 0$ (or, in co-ordinate terminology, $X_{(i;j)} = 0$). V is *geodesic* iff its acceleration $A^i \equiv V^i{}_{;k} V^k$ vanishes. V is *non-rotating* iff its rotation matrix $\omega_{ij}(V) \equiv h_i{}^m h_j{}^n V_{[m;n]}$ vanishes, where $h_{ij} \equiv g_{ij} + V_i V_j$ is the projection tensor. V is *static* iff it is both stationary and non-rotating. V is *parallel* or *covariantly constant* iff $V^i{}_{;j} = 0$. A space-time is said to be stationary (respectively, static) if it admits a global stationary (static) frame.

Lemmas.

We recall a standard lemma relating these definitions to co-ordinate characterizations.

Lemma 1. Suppose that (M, g) admits a stationary frame V. Then for every point $p \in M$ in the domain of definition of V there exists an open neighborhood $N(p)$ and a co-ordinate chart x^i, $i = 1, 2, 3, 4$, covering $N(p)$ such that $V \alpha\ \partial/\partial x^4$ and $g_{ij;4} = 0$. If V is static, then the co-ordinate system can be chosen to have the additional property that $g_{\alpha 4} = 0$, $\alpha = 1, 2, 3$.

Another important but non-trivial lemma about stationary frames was proved recently by Müller zum Hagen.[117]

Lemma 2. A frame V is stationary iff the round trip proper time of a light signal between any two of the trajectories of V is constant in time.

As discussed in section 6, however, lemma 2 is not sufficient to justify interpreting the red shift in a stationary frame as a non-Doppler effect.

Two further lemmas link stationary frames and the red shift.

Lemma 3. Let X be a Killing vector field and P the tangent vector field of a geodesic. Then $P \cdot X$ is constant along the geodesic.

Proof. Let λ be an affine parameter of the geodesic. Then

$$\begin{aligned} d(P \cdot X)/d\lambda &= (P^i X_i)_{;k} P^k \\ &= (P^i{}_{;k} P^k) X_i + P^i P^k X_{i;k}. \end{aligned} \tag{A.1}$$

The first term on the rhs of the second equation vanishes because P is the

[117] H. Müller zum Hagen, 'A New Physical Characterization of Stationary and Static Space–Times,' *Proc. Camb. Phil. Soc. Math. Phys.* 71 (1972), 381–389.

tangent vector of a geodesic. In the second term, only the symmetric part of $X_{i;k}$ contributes to the sum, and this part vanishes because of Killing's equations.

Lemma 4. In a stationary frame V, the photon frequency is independent of the path the photon takes between the source and the receiver; thus one can properly speak of *the* red shift for the frame V.

Proof. Let $V = hX$, where X is a Killing vector field. Killing's equations give

$$V_{(i;j)} = (\log h)_{,(i}V_{j)}. \tag{A.2}$$

Contracting with V^j and using the identities

$$V_j V^j = 1, \; V_{i;k} V^i = 0 \tag{A.3}$$

gives

$$A_i = (\log h)_{,i} + (\dot{h}/h) V_i, \; \dot{h} \equiv h_{,i} V^i. \tag{A.4}$$

Contracting again and using $A_i V^i = 0$ yields

$$\dot{h} = 0. \tag{A.5}$$

So the proportionality factor is constant along the trajectories of V. From lemma 3 and the discussion of section 3, it follows that the photon frequency ratio ν_R/ν_S, for source S and receiver R co-moving with V, is $h|_S/h|_R$. Hence, the frequency ratio is independent of when the photon leaves S and also of the (nonbroken) null geodesic connecting S to R.

Another consequence for the red shift is given in:

Lemma 5. If the frame V is both stationary and geodesic, then there is no red shift for source and receiver co-moving with V.

Proof. Combining (A.4) and (A.5) gives for a stationary frame

$$A_i = (\log h)_{,i}. \tag{A.6}$$

Thus, if V is geodesic as well, h = constant, and the photon frequency ratio is unity.

This lemma does not necessarily mean that there is no red shift for a source S and receiver R which are both at rest in a stationary frame and which are both in geodesic motion. But this consequence does hold if a mild form of Copernicanism is added — namely, S and R are not privileged in that all the other observers at rest in the frame are also in geodesic motion.[118]

[118] The research for this paper was supported by National Science Foundation Grants Soc. 78-01076 and 78-03887.